HALLEY'S COMET

AN EVENING DISCOURSE TO THE BRITISH ASSOCIATION, AT THEIR MEETING AT DUBLIN, ON FRIDAY, SEPTEMBER 4, 1908

BY

H. H. TURNER, D.Sc., F.R.S.

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HALLEY'S COMET, 1066. (From the Bayeux Tapestry)

From Chambers's 'Handbook of Astronomy'

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HALLEY'S COMET

THE British Association is to meet next year at Winnipeg; so that before it again assembles on this side of the Atlantic Halley's comet will have returned once more to perihelion. Its last return was in 1835; and judging by the records of that time, it may be expected to be a fairly bright comet in May, 1910. But it will be by no means one of the brightest comets: it will probably not compare, for instance, with the great comet of 1858, which some of us remember, and which is associated with a famous vintage. The fame of Halley's comet is due to two causes almost independent of its brightness: to its long history (which has now been carried back definitely, by the admirable work of Messrs. Cowell and Crommelin of the Royal Observatory, Greenwich, to B.C. 240), and to the circumstances under which it became associated with the name of Halley, who discovered, not the comet itself, but its periodic character. This discovery—that the same comet might and actually did return to us again and again—followed as a natural consequence from Newton's great discovery of the Law of Gravitation: and the story of Halley's comet thus forms an integral part of the most important event in the whole history of science. The Law of Gravitation was not realized in its complete form at a single epoch: Newton's work began in 1665, and was not matured until 1685: while Halley's deduction was dated twenty years later still (1705). But even forty years is a short period for the unrolling of that great revolution in scientific thought. We shall better understand the magnitude of the change if we first glance at a few records showing the state of affairs immediately preceding.

In the very year 1665, in which Newton, driven from Cambridge by the incipient Great Plague, first began to meditate on Gravitation, there was published the first number of the Philosophical Transactions of the recently established Royal Society of London. And in that first number is an account of a paper by a certain M. Auzout, 'a French

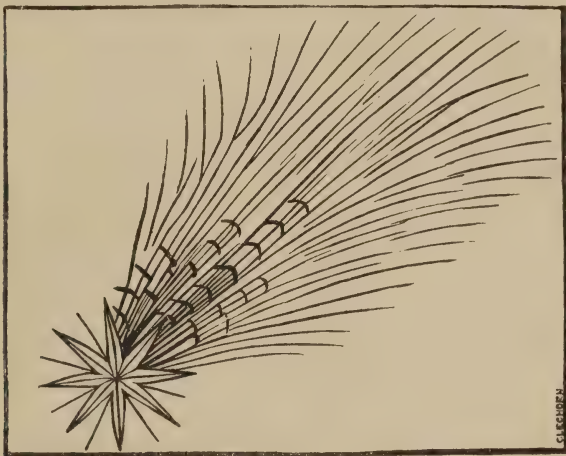
gentleman of no ordinary Merit and Learning,' in which an attempt is made to predict the movements, among the stars, of the comet of 1664. The author proudly refers to his project as 'a design which never yet was undertaken by any Astronomer, all the World having been hitherto perswaded, that the motions of Comets were so irregular, that they could not be reduced to any Laws'. The success attained by M. Auzout was, however, only moderate. He seems to have had an inkling that the orbit lay in a plane, so that the apparent path in the heavens might be a straight line: but his application of this limited knowledge was faulty: and he had no idea at all of the curve traced *in* the plane. He returned to the charge in a second paper on the path of the comet of the following year, 1665; and remarks that the second comet seemed to give a better chance than the first of determining 'the Great Question whether the Earth moves or not'. We are thus reminded how slight was the knowledge of that time, not of the movements of comets only, but even of that of our earth itself. It was *suspected* to move round the sun; but no *proof* of its motion by direct evidence was forthcoming until Bradley (Halley's contemporary at Oxford and successor at Greenwich) measured its velocity nearly a century later.

If so little was known of comets by scientific men, we shall not be surprised to find that the knowledge of the layman was less still: and since that which is not understood is apt to be disquieting or even terrifying, it was natural to regard the mysterious comets as the probable causes of disaster. The notion undoubtedly came from antiquity: but that it had survived the ages we may gather from the following passage in Milton's *Paradise Lost*, which he finished in the same year (1665) in which M. Auzout's first paper appeared.

> . . . on th' other side,
> Incenst with indignation Satan stood
> Unterrifi'd; and like a comet burn'd,
> That fires the length of Ophiuchus huge
> In th' Arctic sky, and from his horrid hair
> Shakes pestilence and war.
>
> (*Paradise Lost*, Book II, lines 706–11.)

There are lines closely similar in Homer and Vergil[1]: but there are clear indications that Milton was not merely using the imagery of a bygone age. He was almost certainly referring to a particular comet which had been very definitely associated with great calamity, viz. the comet of 1618 referred to in Evelyn's *Diary* as follows (under date 1624):—

> and the effects of that Comet, 1618, still working in the prodigious revolutions now beginning in Europe, especially in Germany.



HALLEY'S COMET, 684. (*From the Nuremberg Chronicle.*)

From Chambers's *Handbook of Astronomy.*

It is true that this comet of 1618 appeared when Milton was a boy of ten: but it was in the constellation Ophiuchus, specially mentioned in the above passage; it must have been

[1] [The helmet of Achilles shone]
Like the red star, that from his flaming hair
Shakes down diseases, pestilence and war.
(Pope's Translation of *Iliad*, xix. 381.)

Nec secus ac liquida si quando nocte cometae
Sanguinei lugubre rubent, aut Sirius ardor
Ille sitim morbosque ferens mortalibus aegris.
(*Aeneid*, x. 272.)

a most striking object, the tail being recorded as 104° in length; and it was so definitely held responsible, as Evelyn shows us, for the great Thirty Years' War that the impression made on the mind, even of a boy of ten, may well have lasted until he wrote the above lines as a man of fifty. It has been usual to regard the particular reference, if any, as being to the comets of 1664 and 1665 of which M. Auzout predicted the movements, the former of which was believed to have led to war with the Dutch, and the latter to the Plague of London. But Milton never saw these comets, being already blind: and unless the lines were added in revision, they were probably written before 1664. Moreover, neither of these comets, nor any intermediate comets (of which there were no striking examples), was in Ophiuchus; and the explanation offered of the use of this name because it was 'fine-sounding' is scarcely satisfactory. It is true that Ophiuchus is not 'in th' Arctic sky', being by no means a northern constellation: but these words probably refer rather to the attitude of the poet himself, since Milton continually posed as a classical poet writing from Italy; and may in this case have desired to make it clear that the comet had been seen from England.[1]

That we have forgotten this dread of comets, which seems to have been real enough in Milton's time, is due to two causes: (1) comets have become more familiar to us, and (2) their movements are now well understood.

(1) Numerous discoveries of faint comets, due to the development of the telescope, have shown us that they are by no means rare visitors; and it would be difficult to identify the calamities due to each if we had the wish. Several comets are found every year, both new and old. Since Halley taught us that his comet would return, we have learnt the same of many others: and though they are often missed on their brief visits (owing to uncertainties in prediction, bad weather, and other causes), the successes in recovering them are sufficient to give confidence in accounting for the failures. Many of them, however, never return at all, or if they do, it is only

[1] For this important suggestion I am indebted to Mr. H. E. Butler, Fellow of New College, Oxford.

after so long an interval that it is impossible to be sure of identity. Taking new friends and old together, we have had a curiously steady average of five detected visitors a year during the last thirty years, as the following table shows. That the number of cometary discoveries does not increase at the same rate as that of the minor planets (shown in the same table) is due to the fact that it has not been found possible to utilize photography in the same way for comets as for planets.

NUMBER OF DISCOVERIES.

	Comets.		*Minor Planets.*
	New.	*Old.*	
1878–1887	36	17	92
1888–1897	37	16	190
1898–1907	37	12	501

The great increase in the number of minor planets found in the decade 1888–97 as compared with the previous decade may be set down to the invention of the dry-plate, which made it easy to expose a very sensitive plate to the light of the stars for a long interval. If the telescope is meanwhile kept pointed accurately to one of them, that star, even though faint, will leave an image—a round dot—on the plate : and so will other stars in the neighbourhood, the dots being larger and blacker as the star is brighter ; for the stars are sensibly motionless relatively to each other. But if within the area photographed there happens to be a little planet, moving relatively to the stars, it will make a short line or 'trail' on the plate and thus betray its planetary character. The plate thus forms the simplest kind of 'trap' for catching planets. Only those which are very faint elude it, for the reason that the image of a very faint moving object is never sufficiently long on any one point to impress the plate.

It is, however, possible to modify the trap so that even faint planets may be caught. If we know beforehand the direction in which such a one is moving, and the rate of travel, we can

point the telescope by calculation so as to keep the moving object on one spot, and then its light, even though very faint, ultimately forms an image by accumulated effect. The stars will now become trails, but the planet will be a dot, which can therefore be detected among them as readily as before. This ingenious method is due to Dr. J. H. Metcalf, of Taunton, Mass., U.S.A., and by the use of it he has in recent years greatly extended the number of faint planetary discoveries, being responsible for a large portion of the number 501 for the decade 1898–1907 in the above table.

But it must be remembered that the success of the method depends upon our knowing in advance the direction and magnitude of the movement of the body to be detected. The movements of the minor planets, which belong to a family with strong resemblances, can be thus estimated, at least approximately: but the comets have no such family traits, and the method is not applicable. Moreover, even the simpler method of exposing a plate to the stars and hoping for a comet to betray itself by a 'trail' is not nearly so easy as in the case of planets. Minor planets are nearest the earth (and therefore brightest to our view) when on the side of us remote from the sun, and therefore least interfered with by the glare of his rays. But it is just the reverse with comets. They are brightest when seen near the sun and when it is consequently most difficult to photograph them without sunlight fogging the plate. On rare occasions is this difficulty removed or lessened, namely, when the sun is totally eclipsed, and then the 'comet-trap' may be set in his very face. But even then successful captures have been few. The most famous is that of 1882, when those who photographed the eclipsed sun in Egypt were delighted by the appearance of a beautiful little comet on their plates alongside the corona. Since then the eclipsed sun has been photographed in 1883, 1886, 1887, 1889 (twice), 1893, 1896, 1898, 1900, 1901, 1905, 1907; but only once has a comet appeared on the plates (1893), and even that was so faint as to escape the vigilance of every one but Professor Schaeberle, who had indeed hard work to convince us of his discovery. So that even at total eclipses we cannot

hope to find many comets by photography: though these two instances hint that many come and go without being detected; for the total time during which the sun was eclipsed on these thirteen occasions only amounted to half an hour. We cannot hope to detect all the faint comets that visit us: and for such as we may hope to find we must trust to the vigilance and assiduity of observers, rather than to the machinery of photography.

The eclipse photographs of 1882 were the first photographs to be taken of a comet. But in view of their accidental character, precedence is usually accorded to those taken a few months later of the Great Comet of 1882. These were taken at the Cape of Good Hope under the auspices of that vigorous astronomer, Sir David Gill, the retiring President of the British Association. They led to most important consequences, chief of which was the international enterprise for photographing the whole heavens—'the Astrographic Chart', as it is called. The comets of 1882 form a most interesting group in every way; the following is a list of them:—

THE COMETS OF 1882.

Date.	*Discoverer.*	*Place.*	
(*a*) Mar. 17	Wells	Albany, N.Y.	Discovered 'to order'
May 16		Egypt	During total eclipse
(*b*) Sept. 3		Auckland, N.Z.	Great Comet
(*c*) Sept. 13	Barnard	Nashville, Tenn.	
Oct. 9	Schmidt	Athens	Companion to (*b*)

We have spoken of the Eclipse Comet of May 16, and of the Great Comet, which appeared so suddenly that several observers saw it simultaneously and no individual can be credited with the discovery. Very different was the case of comets (*a*) and (*c*). The former was the result of a deliberate and painstaking search undertaken in illustration of a great principle, the principle, namely, that an astronomer may be doing most important work without looking through a telescope at all. He may be discussing observations already made, which requires more skill and certainly more labour and

time than is generally known: and we shall presently see how work of this kind conducted Halley to the great discovery which is the central topic of this lecture. In the case immediately before us, Professor Boss of Albany was engaged in a most important discussion of this kind—work which has made him famous; but work which the Board of Visitors of the Observatory were scarcely in a position to appreciate. They inquired tentatively whether it would not rather add to the reputation of the Observatory if some discovery, such as that of a comet, could be made; and were promptly informed that nothing was easier if they would sanction the devotion of a certain sum of money to the purpose, as salary for a person of average intelligence while making the necessary search. The challenge was accepted on the spot; the money subscribed; the searcher set to work, and within the allotted time a fine comet was found. Professor Boss undoubtedly took a certain risk in undertaking to catch a comet, just as a man would who undertook to catch a fish within a definite time. But he was anxious to vindicate his views of the relative importance of different kinds of work, and deserved the success he ventured to count upon.

Comet (*c*) was discovered by Professor Barnard, who has more of these discoveries to his credit than any other observer. In his early days, before he had any definite astronomical appointment, the rewards for such discoveries were of some importance to him; and though they were far from providing a regular income, the very fact that they can be mentioned in the same phrase illustrates in a different manner the part played by assiduity in the discovery of comets. Finally, the last comet on the list was seen to be a companion to comet (*b*), moving in the same orbit. It seems probable that it once formed part of the same body; and we are thus reminded of the disintegration of comets, to which we shall return later. It is not in my power to explain how these discoveries are made, but that ingenious and accomplished artist, Mr. George Morrow, has made it possible to show at a glance how they are *not* made. A triumph of his art appeared in *Punch* for Dec. 5, 1906, entitled 'Discovery of a Comet at Greenwich Observatory,

DISCOVERY OF A COMET AT GREENWICH OBSERVATORY

By our Untrustworthy Artist in London

From Punch for Dec. 5, 1906, by kind permission of the Proprietors of Punch

by our Untrustworthy Artist in London', and by the kind permission of the proprietors of *Punch* I am able to recall it to your memory here. In the background is a good representation of the oldest part of Greenwich Observatory, the 'Octagon Room', designed by Sir Christopher Wren in 1675; while to the front is the recently erected dome for the 28-inch refractor, which those familiar with the Observatory buildings will recognize as faithfully represented. But almost everything else is to be interpreted by the rule of contrary; the telescope, for instance, does *not* extend out of the dome, which was expressly designed in the shape it possesses in order to contain the new telescope of 28 feet focal length, without such projection. The assembly of such a crowd of astronomers outside the observatory similarly indicates that there is no one at all, astronomer or not, likely to be at such a time in the Park, which is closed at night. But it is not difficult to read the delightful picture without the assistance of comments.

(2) Our dread of comets has further been dissipated by our knowledge of their regular movements round the sun, in obedience to the same Law of Gravitation which controls the movements of the planets and of our Earth itself; so that many of them are regular members of the Solar system. Their orbits differ from those of the planets in being far more highly elliptic. Our own path round the sun is nearly a circle, so that our distance from him remains nearly the same all the year round; but the distance of a comet from the sun varies greatly, from perihelion when he is near and consequently bright, to aphelion when he is so distant and faint that we lose sight of him; indeed, we have generally lost sight of him long before. Sometimes there is no aphelion at all. If the orbit is really elliptic, then there at last comes a time, though it may only be after thousands of years, when the comet's journey away from the sun comes to a turning-point, and he again begins to draw nearer: the turning-point is called aphelion. But the conditions may be such that this never occurs—that the comet goes indefinitely further away from our sun until he is caught by some other star, and his orbit—no longer elliptic, but parabolic or hyperbolic—changes

into a new parabola or hyperbola with the new sun as attracting centre.

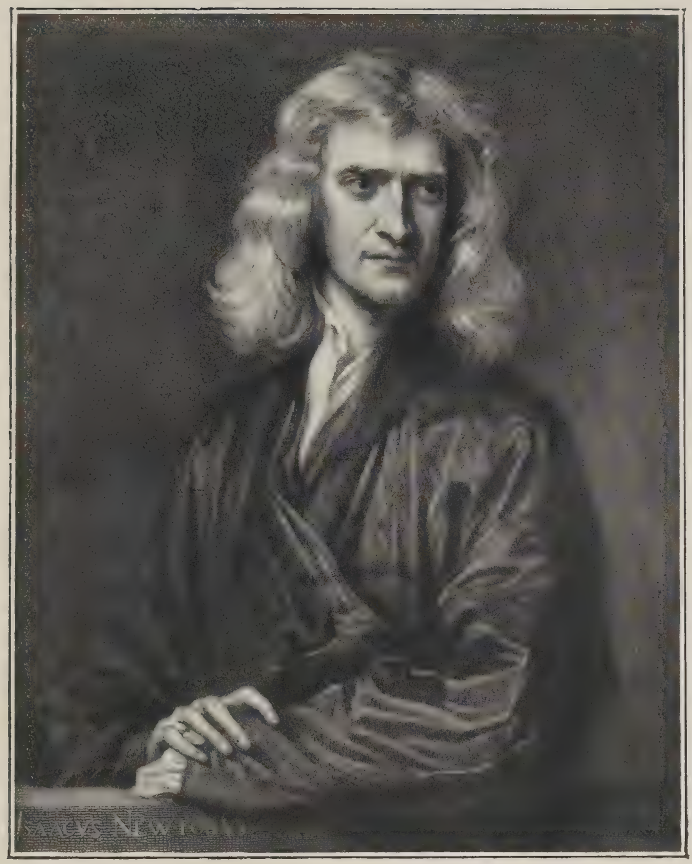
This explanation of cometary movements followed at once on the establishment of the Universal Law of Gravitation by Newton, and therefore dates essentially from the publication of the *Principia* in 1686. But so little was it anticipated that even Newton himself denied one of its obvious corollaries so late as 1680. There was a fine comet in that year which disappeared at perihelion into the glare of the sun's rays, and reappeared on the other side of the sun, in a manner now familiar to us. Flamsteed, the first Astronomer Royal, identified the two appearances as being of the same comet: but others reckoned them as different comets; and when Flamsteed wrote to Newton suggesting the identity, Newton was politely incredulous. This seems all the stranger when we remember that Newton had at that time had the vague suggestion of the Law of Gravitation in his mind for some fifteen years. The fact is that two important propositions remained to be proved, without which the general idea was barren; so that in 1680 there was no suspicion in his mind of the real truth about comets.

It was just at this time (end of 1679 or 1680) that his thoughts were drawn back to the subject of gravitation by a correspondence with Hooke, and that he entered upon the work which he alone was capable of doing. There were three distinct steps in the establishment of the Great Law which may be stated as follows:—

First Step.

1665 or 1666. *Kepler's Third Law connecting the periods of the planets with their distances from the sun suggested to Newton an attractive force in the sun varying inversely as the square of the distance.*

(The same idea occurred later, but independently, to others. In this first step Newton had merely priority.)



NEWTON

Second Step.

1679 or 1680. *Correspondence with Hooke suggested that under such a force a particle would describe an ellipse with the sun in focus. Newton proved it.*

(Others, including Hooke, thought of this; but no one could prove it. Newton succeeded where others had tried their best and failed. But the correspondence with Hooke irritated him, and he did not even mention his success.)

Third Step.

1685. (Halley visited Newton at Cambridge in August, 1684, specially to ask whether he could prove the above proposition, and learned to his delight that it had been proved. Under Halley's enthusiastic encouragement he proceeded to work further at the subject and in 1685———)

Newton proved, without a vestige of outside suggestion, that a sphere would attract as though concentrated at its centre, even at points close outside its surface.

(Without this wonderful proposition, the necessity of which seems never to occur to any one but Newton, the application of the Law of Gravitation to the movements of the heavenly bodies would have been a mere rough approximation.)

Even in this brief tabular statement we get some indication of the importance of Halley's influence: and indeed its importance can scarcely be over-estimated. It seems quite possible that but for Halley we might never have learnt the truth from Newton, who was shy and sensitive, and so heartily disliked the controversies which seemed to follow the announcement of his discoveries, that he more than once was on the point of abandoning the pursuit of philosophy—'a litigious lady' who seemed bent on getting him into trouble.

Particularly had he suffered already in this way at the hands of Hooke; and so when irritated into proving, almost in spite of himself, the proposition about the ellipse which we have called the Second Step, he did not send Hooke his proof, but

tossed it carelessly aside—so carelessly that he could not find it again to show Halley in 1684. The course of events was as follows:—Hooke wrote to Newton sending some gossip about scientific matters (for example, the news that Flamsteed had detected a large parallax in the stars, thus confirming Hooke's own previous results: both observations afterwards turned out erroneous) and asking for similar news in exchange. Newton replied[1] that he had not had time for philosophical studies lately, but could suggest an experiment to demonstrate the earth's rotation, viz. if a body were let fall from a tower it should fall to the east of the vertical, 'contrary to the opinion of the vulgar, who think that if the earth moved heavy bodies in falling would be outrun by its parts and fall on the west side of the perpendicular.' He drew a diagram to illustrate the path of the falling body, and, for some reason not quite easy to assign, continued the path down into the earth as a spiral line to the centre. Hooke jumped at the opportunity of airing his claim to have proved the 'proposition of the ellipse'—a claim he often put forward but never substantiated—and pointed out that Newton's spiral should have been an ellipse, as he (Hooke) could prove. Now, to be aggressively corrected is always a little trying, even when the corrector is entirely in the right and his victim entirely in the wrong. But in this case Hooke was exasperatingly in the wrong himself. The elliptic path was not in question: the path drawn by Newton was in any case *relative to the rotating earth*, and not the absolute path in space. If we assume with Hooke that the particle was attracted by the law of the inverse square, we find a *relative* path very like that drawn by Newton, except near the centre. But again, the law of the inverse square would not hold within the earth in any case; and at that time Newton did not believe it held even near its surface. He had not yet made the most important Third Step in the proof; and he wrote to Halley on June 20, 1686, 'I never extended the duplicate proportion lower than to the superfices of

[1] This letter (of November 28, 1679) was lost, but was recovered about twenty years ago, and is now in the Library of Trinity College, Cambridge. See Note A.

the earth, and before a certain demonstration I found the last year, have suspected it did not reach accurately enough down so low.' We can therefore well understand the manner in which Newton writes, in this same letter to Halley, of his correspondence with Hooke:—

In my answer to his [Hooke's] first letter I refused his correspondence, told him I had laid philosophy aside, sent him only the experiment of projectiles (rather shortly hinted at than carefully described) in compliment to sweeten my answer, expected to hear no further from him; could scarce perswade myself to answer his second letter; did not answer his third, was upon other things; thought no more of philosophical matters than his letters put me upon it, and therefore may be allowed not to have had my thoughts of that kind about me so well at that time.

In justice to Hooke we must remember that he stimulated Newton to prove the proposition of the ellipse, though the stimulation was of so disagreeable a kind that Newton put aside his proof, as above stated, rather that risk further irritation by continuing the correspondence with Hooke.

There could scarcely have been a greater contrast than that between Hooke and Halley, or between the effects the two men produced upon Newton. Hooke's aims were so clearly to call attention to his own merits: Halley's to exalt those of others, and to secure the fruits of their work for the world. He too, like Hooke, felt sure of the truth of the great proposition about the ellipse; but he knew well enough that the proof of it was beyond him, and he had applied in vain for it to Wren and others, including Hooke himself. As a forlorn hope he made his immortal journey to Cambridge (when did ever an Oxford man take a journey to Cambridge of such vital importance to the world!) in August, 1684, expressly to ask Newton whether he could supply the proof; and to his surprise and delight learnt that his hopes had been realized some years before. The rest of the story has many times been told—how Newton failed to find the manuscript, but promised to send it later; how he ultimately sent a new proof, with much other new matter which rapidly developed into the *Principia*; how when that immortal work was presented by Halley to the Royal Society, and they ordered it to be printed, it was found that they had no funds wherewith

to proceed, having spent so much on a great work on Fishes that they were actually paying the salaries of their officers in copies of that work; and how Halley, though now a poor man with a family to support, having once been a rich man and retaining the rich man's contempt for financial difficulties, paid for the printing out of his own pocket. Well was he entitled to claim, as he proudly did on more than one occasion, to be the 'Ulysses who had produced Achilles'.

It was therefore eminently appropriate that one of the earliest fruits of this great discovery should fall to Halley, whose generous enthusiasm had been so important a factor in the production of the *Principia* to the world. But he was by no means a passive recipient of this good fortune: the labour he undertook to earn it was exceptional even in the history of scientific discovery. When in 1704 he was appointed Savilian Professor of Geometry at Oxford, he calculated parabolic orbits for as many comets as he could find sufficiently well observed, to the number of 24, and published the results in his classical *Astronomiae Cometicae Synopsis*, which was first printed in the Philosophical Transactions for 1705. Even at the present date a work of this magnitude would necessitate great labour, and with the imperfect appliances and knowledge of the time the toil involved was such that its accomplishment in so short a time is an almost incredible achievement. Moreover, it does not seem probable that when he set out on this gigantic task Halley had any expectation of the reward which he actually obtained: he was moved simply by his enthusiasm for the great discovery of Newton, and by his joy in using it to some purpose in calculating orbits for those mysterious comets, which less than half a century before 'all the World had been perswaded' (according to M. Auzout) 'could not be reduced to any Laws'. However, as often happens in scientific work, the most important outcome was unexpected. He found that among his calculated orbits there were three which were almost identical. The elements were approximately as follows :—

TABLE I

Date of Comet.	*Interval.*	*Longitudes of*		*Inclination.*	*Distance.*
		Node.	*Perihelion.*		
	Y. M.				
1531, Aug. 24		49°	301°	18°	0·57
	76 2				
1607, Oct. 16		50°	302°	17°	0·59
	74 11				
1682, Sept. 4		51°	302°	18°	0·58

It will be seen that in the last four columns of the table the numbers are very nearly the same. It would not be surprising to find close similarity in one or even two of these columns, but to find it in all four suggested something more than mere coincidence. Perhaps this will be clearer if we look at a part of the table from which these three orbits have been selected, in order to see the average agreement or disagreement.

TABLE II

PART OF HALLEY'S LIST OF TWENTY-FOUR ORBITS OF COMETS

Year.	*Longitudes of*		*Inclination.*	*Distance.*
	Node.	*Perihelion.*		
1337	84°	38°	32°	·41
1472	282°	46°	5°	·54
1531	49°	301°	18°	·57
1532	80°	111°	33°	·51
1556	176°	279°	32°	·46
1577	26°	129°	75°	·18
Four omitted.				
1607	50°	302°	17°	·59
1618	76°	2°	38°	·38
1652	88°	29°	79°	·85
1661	82°	115°	33°	·45
&c.	&c.	&c.	&c.	&c.

In comparison then with the average differences of Table II we may certainly regard those of the last four columns of Table I as slight though still worthy of attention: but what

arrests the attention even more is the difference between the two intervals in the second column of Table I. If it were really, as Halley rightly conjectured, the same comet which had appeared in 1531, 1607, and 1682, why had it not returned after exactly equal intervals? The regularity of the earth's revolution round the sun was a familiar fact: one year is equal in length to another, and a year is simply the interval between two returns of the earth to the same spot. And so also the intervals between returns of the planets were exactly equal—but stay! was that after all quite true? Halley remembered that a deviation from equality had been noticed in the case of the planets Jupiter and Saturn; and he had himself guessed with great acuteness at the cause of it. He had divined that the 'great inequality' was due to the attraction of these planets one for the other, acting in addition to the attraction of the sun for each; and he inferred that the inequality in the two intervals of the comet's return might be due to a similar cause. It was true that the differences were, in the case of the planets, only a few days, while that for the comet was many months: but then the comet's orbit was much more easily disturbed. An increase of the velocity by little more than one per cent. would send the comet away into space never to return at all; and hence a very slight perturbation would suffice to delay it by many months. In fact he saw no difficulty which could not be explained away in concluding that the three sets of elements in Table I referred really to the same comet; and he predicted that it would again return in another seventy-five or seventy-six years, say in 1758 or thereabouts. This return he could not himself hope to witness (he died in 1742 at the ripe age of eighty-six), but he trusted posterity, when the comet did reappear, to credit an Englishman with the prediction. 'Quo circa si secundum predicta nostra redierit iterum circa annum 1758, hoc primum ab homine Anglo inventum fuisse non inficiabitur aequa posteritas.'[1]

[1] These words are not in the original paper, but were added in a later edition, which, however, was not published until after Halley's death, in 1749.

There can be no more complete or more sensational proof of a scientific law than to predict events by means of it. Halley was deservedly the first to perform this great office for Newton's Law of Gravitation: and he would have rejoiced to think how conspicuous a part England was to play in the subsequent prediction of the existence of Neptune.

Before the reappearance of the comet was due there was ample time to make calculations of the effects which Jupiter and Saturn would actually produce, though the planets Uranus and Neptune, which also contributed something to the perturbation, were as yet undiscovered. These calculations foreshadowed a greater delay than had been anticipated, and the comet did not return until 1759. But the delay, the causes of which Halley had so expressly recognized, really added fresh laurels to his success in prediction. The comet went round once again and reappeared in 1835; and now we are eagerly expecting its return in 1910. Calculations of the circumstances of return have meanwhile been made chiefly by foreign astronomers, the name of the great Frenchman Clairaut being specially associated with the calculations for 1759: but in the last year or two Messrs. Cowell and Crommelin, of the Royal Observatory at Greenwich, have done splendid work of this kind, calculating by improved methods, specially devised for the purpose, the action of all the known planets so accurately that if the comet does not return at the time they predict, we may take it as very probable that there is some unknown disturbing cause.

But we will recur to their work presently. There are still one or two points worthy of remark about Halley's discovery; and especially we may ask how it came about that he waited nearly twenty years before working out the twenty-four orbits which led to it? The main facts about gravity were known to him in 1685, and yet it was not till 1704 that he applied his knowledge and enthusiasm to the case of comets.

We may pause a moment to reflect how rapid would have been the revelation if he had undertaken the work, say before 1690: in 1680 Newton himself did not realize the reappearance of a comet at the same perihelion passage; within ten years

the regular return of comets for centuries might have been established by Halley as a consequence of Newton's discoveries. But it actually took thirty years instead of ten. What was the reason?

The answer is that Halley was a man of many enthusiasms; and among other things he had been roaming the world in the hope of solving another great problem of the time, that of finding the longitude at sea. Nowadays the sailor finds his longitude by carrying with him a chronometer indicating Greenwich time; for it is now possible to construct chronometers which perform with marvellous accuracy, in spite of great ranges of temperature or other alterations of conditions liable to occur in a long voyage. But in the seventeenth century the chronometer had not been invented; and the alternative method of 'lunar distances' (lately superseded by that of the chronometer) was not available owing to lack of knowledge of the moon's movements. The problem of finding longitude was one of immense difficulty and immense importance; and any hope of solving it was eagerly caught at. It seemed possible that the deviation of the compass from true north might be a guide; for this deviation varies with the longitude. Accordingly Halley set out on a long voyage to investigate the manner in which it does vary with longitude. It tells us something of his extraordinary powers that he was actually put in command of one of H.M. ships, the pink *Paramour*, and commanded her through all the troubles of a long voyage, including mutiny of his first lieutenant. His commission is still on the Admiralty minutes, and runs as follows:—

(From *Speculum Hartwellianum*, p. 9.)

INSTRUCTIONS TO CAPTAIN EDMUND HALLEY,
COMMANDER OF HIS MAJESTY'S PINK, THE PARAMOUR.
Dated October 16, 1698.

Whereas his Majesty has been pleased to lend his Pink the Paramour for your proceeding with her on an expedition to improve the knowledge of the Longitude and variations of the Compasse, which shipp is now

completely Man'd, Stored, and Victualled at his Majesty's Charge for the said Expedition:—

'You are to make the best of your way to the southward of the Equator, and there to observe on the East Coast of South America, and the West Coast of Affrica, the variations of the Compasse with all the accuracy you can, as also the true situation both in Longitude and Latitude of the Ports where you arrive.

'You are likewise to make the like observations at as many of the islands in the seas between the aforesaid Coasts as you can (without too much deviation) bring into your Course; and if the season of the year permit, you are to stand soe farr into the South till you discover the Coast of the Terra Incognita, supposed to lye between Magolan's Streights and the Cape of Good Hope, which Coast you are carefully to lay down in its true position.' &c., &c., &c.

Nevertheless there had been one possibility of Halley being in a position to undertake his calculations on comets at an earlier date, namely, when the Savilian Chair of Astronomy[1] fell vacant in 1691. Halley was an eager candidate, and we can scarcely doubt that one of his reasons was a wish to have leisure for those researches which were by adverse Fate deferred until 1704. For he was not elected. His rejection has been ascribed to his heterodox religious opinions, and with some show of probability. Half a century ago it was considered necessary to write an essay[2] defending him from the charge of religious infidelity; and we gather that if such charges were made, they rested on such grounds as that he had speculated on the age of the sea, which he estimated by its rate of growing more salt, and its present state. In such estimations Halley was anticipating by a century or two the thoughts of some of our greatest scientific minds to-day; but to those who regarded the Biblical 4,000 years as unquestioned and unquestionable, his enterprise may have looked rather alarming. It must be remembered, however, that the alarm, if real, was unlikely to have subsided in 1704, when Halley was elected without any such difficulty to the Chair of Geometry; and it seems more probable that

[1] See Note B for a list of the Savilian Professors.

[2] *A Defence of Halley against the charge of Religious Infidelity*, by the Rev. S. J. Rigaud, M.A., F.R.A.S., &c., Tutor of Exeter College: Oxford, printed for the Ashmolean Society, 1844.

the Electors simply chose in David Gregory one whom they believed to be a man better suited to the needs of Oxford. David Gregory was an able man, one of a great family of able men,[1] descended from an able woman; a family which gave the world the Gregorian telescope, 'Gregory's examples,' and Gregory's 'powder'. Newton himself, it is recorded, favoured the claims of David Gregory in 1691; and we have thus fair cause to think that the omission to elect Halley was no miscarriage of justice; we may be content that his claims were after all recognized in 1704, and his great work accomplished in due season.

A few particulars of Halley's life may be added here. He was born in 1656, and was educated at St. Paul's School and at Queen's College, Oxford. He plunged into scientific work at once by voyaging to St. Helena at the age of twenty, and laying the foundations of southern astronomy in the years (1676–8) of his residence there; and was thenceforward continuously in the front rank of scientific enterprise, observing comets and transits of Mercury, ascending mountains to test the methods of finding heights by the barometer, adjudicating quarrels between men of Science, Secretary to the Royal Society (1685–93), and, as we have seen, publishing[2] Newton's *Principia* at his own expense, or taking command of a ship to find the longitude. In 1704 he settled as Professor of Geometry at Oxford: in 1721 he was appointed Astronomer Royal (without, however, vacating his professorship), and he died in 1742. Such facts are easily recorded; but it is not easy, perhaps it is impossible, to convey in a few sentences any adequate notion of the character of the man such as may be gained by a closer study of his life and works. His passionate admiration for greatness in scientific work, which was manifested so clearly in 1685, found almost equal expression in his closing years in an anxiety that Bradley should succeed him as Astronomer Royal. He offered at one time to resign in Bradley's favour, though failing health (he was partially paralysed towards the

[1] See Note C on 'The Academic Gregories'.

[2] Our admirable *Dictionary of National Biography* uses the phrase 'originated (by his suggestions) Newton's *Principia*'.


Edmundus Halley. Soc. Reg. Soc.

close) intervened to prevent the proposal being carried into effect. It tells us something of his vigorous human nature that he incidentally founded the Royal Society Club. When he came up from Greenwich to the meetings of the Royal Society, he liked to dine with his friends; and they collected round him, heads were counted, and so many pounds of fish bought (fish because Halley had then no teeth and could eat nothing else), which the company proceeded to eat: and so (in 1731) was initiated the Club which has by this time so notable a history.

They then agreed to go to a house in Deans Court between an Alehouse and a Tavern, now a Stationer's shop, where there was a great Draft of Porter, but not drank in the House. It was kept by one REYNELL. It was agreed that one of the Company should go to Knights and buy fish in Newgate Street, having first informed himself how many meant to stay and dine.

The Ordinary and Liquor usually came to half a crown, and the Dinner only consisted of fish and pudding. Dr. HALLEY never eat anything but Fish, for he had no Teeth. . . .

About 1737 he (HALLEY) was seized with a paralytic disorder in his right hand, which, it is said, was the first attack he ever felt on his constitution; however, he came as usual once a week, till within a very short time of his death, to meet his friends in town on Thursdays, the day of the Royal Society's meeting, at what is still called Dr. HALLEY'S Club. His paralytic disorder increasing, his strength gradually wore away, till he expired Jan. 14, 1742, in the 86th year of his age.

Nearly a century after Halley's death a fine piece of work in continuation of his great discovery was accomplished by Mr. J. R. Hind,[1] who, by examining old records and especially the Chinese Annals, was able to indicate with fair probability the following previous appearances of Halley's Comet:—

PROBABLE EARLY RETURNS OF HALLEY'S COMET (HIND)

A.D. (1682)	1223	760	295
(1607)	1145	684	218
(1531)	*1066*	608	141
1456	989	530	66 A.D.
1378	912	451	12 B.C.
1301	837	373	

This notable piece of historical research has in the last two years received valuable additions from Messrs. Cowell and

[1] See *Monthly Notices, R.A.S.*, vol. x, p. 51.

Crommelin, of the Royal Observatory, whose work on Halley's Comet in connexion with its expected return has already been mentioned. These gentlemen have made several corrections to the list, and have rendered as certain as possible under the circumstances what Hind only indicated as probable. They have patiently calculated the perturbations of the planets between each one of its returns, starting with those nearest our own times, and thus using each appearance as a stepping-stone to make sure of the previous one. In this way, by great labour (in which they have gratefully acknowledged the assistance of three volunteers—Dr. Smart, Mr. F. R. Cripps, and Mr. Thomas Wright), they have 'underpinned' the edifice and given it a sure foundation of some two dozen consecutive returns. They found that Hind was wrong in 912 (four months too early), and in 1223 (ten months too late) which should be 1222. This comet of 1222 was a bright one, seen both in Europe and China, but its identity with Halley's was not suspected until this careful investigation proved it. In addition to these corrections, Mr. Crommelin wrote to the present lecturer on July 28:—

> We have now carried back Halley's Comet to B.C. 87 (August) with certainty (one revolution earlier than Hind's list), and with fair probability to B.C. 240 (May). Before this observations are completely wanting. Hind is one and a half years too late for his 608 A.D. return (it really was 607 March), but all his earlier returns are right up to the beginning of his list (−11=B.C. 12). We find 1910 April 12·9 for the next passage, but are going over the work again by a new method.

The fact that Hind should have been wrong in several terms of his series shows how necessary was the patient work of these two calculators. Halley would have rejoiced to learn that the history of his comet had been thus recovered by Englishmen! (Mr. Crommelin is an Irishman: but a meeting of the British Association in Dublin is no occasion for laying stress on such a detail.) And he would no doubt have been impressed by one of the dates in the list, that of 1066, which associates the comet with a decisive epoch in our history. The comet of 1066 appears upon the Bayeux Tapestry (see Frontispiece), and was credited with an

influence on the Norman Conquest of England, so that it is almost a national comet; and Halley's joy that an Englishman was the first to detect its periodic character would have been accentuated had he known this.

There is also an association of the comet with another important event, which Hind thought possible, though certainty is beyond our reach. Hind gave 66 and 65 as alternative dates for one of the returns. Bright comets appeared in both these years, and he was not prepared to choose between them. Calculations have now fixed the comet of 66 as Halley's, which was perhaps therefore the 'sword'[1] which foretold the destruction of Jerusalem, according to Josephus; who rebukes his countrymen for listening to false prophets while so notable a sign was in the heavens.

What of future returns? Can we expect reappearances of the comet to continue indefinitely? Our knowledge of the nature and history of comets, though it has advanced rapidly in the last few decades, is still scarcely sufficient to enable us to answer with confidence: but the indications are that comets are continuously being disintegrated and are ultimately broken up, perhaps into a swarm of meteors. The tail of a comet probably represents its losses at the moment. The tail or train does not, as might be supposed, follow behind the head in the same path, as the smoke follows an engine: it is as often in front of the head as behind it. The tail always, in fact, points away from the sun, as though a strong current of air were blowing in all directions outwards from the sun, determining the direction of the tail as the wind determines that of a streamer. And there is actually this in common between the cause of the tail and a current of air—that both have a tendency to drive away lighter particles from heavier. We blow away chaff from grain: and the fierce *light-pressure of the sun* (to which many astronomers now attribute the formation of cometary tails) in the same way separates the lighter constituents of the comet and drives them outwards into space. Possibly we are wrong in assigning such large

[1] Josephus writes:—What shall we say to the comet that hung over Jerusalem for one whole year together, in the Figure of a Sword?

powers to light pressure—the older view that the repulsive action is electrical may turn out to be more correct—but that will not alter the nature of the separating action, which depends on the fact that the repulsion varies as the surface of a particle, and therefore as the square only of its linear dimensions, while its mass varies as the cube. By reducing the dimensions we thus give the repulsion greater relative importance; halve the size of a particle and it is twice as easy to blow away, halve it again and the facility is again doubled, and so on; and this is true whether we are concerned with light-pressure or electrical action, or the blowing of dust.

Comets thus tend to grow smaller. The losses represented by the tail are difficult to estimate quantitatively: we know neither the volume of the outward stream nor its velocity as yet, though we are hopeful of learning something of the velocity before long; but we feel sure that it has volume and velocity, and hence that the loss cannot go on indefinitely. The action is probably only serious when the comet is near the sun—in the distant parts of its orbit the tail may be insensibly small; but at each successive return to perihelion the sun will exact its relentless tax, until there is no more to exact. Doubtless there is a certain process of renewal going on simultaneously: the fierce heat of the sun acting upon the heavier constituents of the comet will melt and volatilize portions of them, and as a result new and lighter constituents will be formed, to replace those driven off into the tail. But this only carries the process of dissipation deeper into the body of the comet and makes the ultimate disintegration more complete. It is always going on—not only in the extreme form rendered visible in the tail, but also as a slow dispersion of the heavier constituents according to their size—so that small portions of matter which once formed part of the comet's head may in time be encountered at a considerable distance from it, and are actually so encountered by our earth in the form of meteors. The establishment of the close association between comets and meteors is one of the scientific achievements of the last forty years,

dating from the great meteor shower of 1866. The orbit of the swarm which caused the shower was identified with the orbit of a comet, and other examples were soon forthcoming of a connexion between comets and meteors. Accordingly we may look to the probable end of Halley's Comet as being dispersion into a swarm of meteors, some of the outlying members of which our earth may encounter at times, and so be lit up for a few moments by the expiring fragments of what was once a terrifying comet.

But that end is not yet. Judging by the circumstances of recent returns, the comet is still vigorous and should be a bright object in 1910, especially in May; though, as remarked at the outset of this discourse, it will be by no means such a magnificent comet as Donati's in 1858. An ephemeris was published by Messrs. Cowell and Crommelin in March last and the following extracts from it will serve to indicate generally the circumstances of the next return :—

Greenwich Noon.	*Right Ascension.*	*Decl.*	Δ.	*Brightness.*	*Sun's Place.*	
					RA.	*Dec.*
	H. M.				H. M.	
Jan. 2	1 42	$+9.5^\circ$	1·3	1·0	18 49	-23°
Feb. 3	0 30	$+6.2^\circ$	1·7	1·1	21 5	-17°
Mar. 3	0 0	$+5.5^\circ$	1·8	2·0	22 54	-7°
Apr. 4	23 26	$+4.2^\circ$	1·4	9·0	0 51	$+5^\circ$
May 2	23 36	$+2.5^\circ$	0·4	58·3	2 35	$+15^\circ$
May 6	0 6	$+2.6^\circ$	0·3	126·5	2 50	$+16^\circ$
May 10	2 3	$+3.0^\circ$	0·1	1112·0	3 6	$+17^\circ$
May 14	7 22	$+0.6^\circ$	0·1	660·7	3 22	$+18^\circ$
May 18	9 10	-0.7°	0·2	94·2	3 37	$+19^\circ$
May 22	9 44	-1.2°	0·4	33·3	3 53	$+20^\circ$
May 26	10 0	-1.3°	0·6	15·5	4 9	$+21^\circ$
May 30	10 10	-1.5°	0·7	8·6	4 26	$+22^\circ$

It will be seen from the fifth column how sudden is the increase of brightness in May. The dates may be generally too late or too early, by even so much as a week possibly; but the order of events will not be greatly altered, and we may expect the comet to be very bright some time in May. Of course it will be then near the sun, as is the way of comets, and therefore only visible near sunrise or sunset :

and unfortunately for us in the Northern Hemisphere, it will be south of the sun after March, so that the comet will not show to advantage above our northern horizon. Our friends in the Southern Hemisphere will be better situated.

It is possible that to some of them a sensational view of the comet will be vouchsafed, though Mr. Crommelin tells me that the most recent calculations render that event less probable than was at one time expected. There will be a total eclipse of the sun on May 8, 1910, just when the comet is very bright, and it may be possible to see Halley's Comet during the total phase, when the blinding glare of the sun is for a few minutes cut off. If so, it should provide a magnificent spectacle, for these are the most favourable of all conditions for seeing a comet. Usually there is a balance of advantages: a comet becomes brighter and bigger as it approaches the sun, but at the same time it suffers more and more (to our view) from the sun's glare, diffused by our atmosphere. At a total eclipse the sun's glare is cut off before it reaches our atmosphere, and the full brilliance of objects usually hidden from view can be enjoyed. Thus we see the sun's own wonderful corona, never seen at other times: and it is for the study of the corona that astronomers travel thousands of miles on these rare occasions of a total eclipse. They hope also, perhaps, by diligent scrutiny of the vicinity to discover a possible planet close to the sun—within the orbit of Mercury—though negative results up to the present indicate that if such a one exist it must be very small and faint indeed. And, similarly, comets may be discovered near the sun on such occasions (as at the eclipses of 1882 and 1893 already mentioned), though they may never be seen before or after. And finally, a really bright comet, such as Halley's, seen imperfectly at other times, should be at its grandest splendour.

What is the reason of the doubt about this event? It is due simply to the scarcity of land in the Southern Hemisphere. The track of eclipse runs across the ocean, and only at its north-eastern extremity fringes Tasmania. A hardy

mariner, who should care to take up a suitable position on May 8, 1910, and was favoured with a cloudless sky, would be sure of a magnificent view of the comet during totality. But an astronomer who wishes to set up an instrument on *terra firma* must be in Tasmania ; and there it seems possible that the comet may have set below his horizon before totality is reached. We shall know more of the chances when the comet is found again, as we hope, in the coming autumn. We are in the position of a traveller expecting a train. We know the line on which it will travel, but it may be punctual or late, according to the unknown circumstances of its distant journey. When the train is still afar off we get by telegram news of its punctuality or lateness, and can then infer at what moment it will come rushing up to the platform in full glory. And so with a comet. Its journey may have been delayed in distant space by circumstances unknown to us, but the sensitive photographic plate should give us news of it while still far away, news whether it is late or (unlike a train) perhaps early ; and we can then tell when it will rush round the sun in full splendour, and what are the chances that our friends in Tasmania will have the sensational view of it during eclipse. Surely we may hope that our national comet will extend this favour to one of our Colonies?

And it could scarcely be called selfishness to indulge in the pious hope that the good fortune of first detecting the comet should fall to an Englishman. Its return can scarcely fail to turn the thoughts of Englishmen to the memory of that great man who was proud to do so much, not only for England, but for the enlightenment of the whole world.

NOTE A.

NEWTON'S LETTER OF NOV. 28, 1679.

This most interesting letter was, by the kind permission of the Master and Fellows of Trinity College, photographed for purposes of reference in this Discourse by Mr. W. F. Dunn of the University Library, and portions of it thrown upon the screen in facsimile. There is no doubt about the 'spiral', which is quite deliberate and not due to a 'negligent stroke of the pen', as had been stated (see Brewster's *Life of Newton*). But as it is hoped that the whole letter may shortly be published, there is less need to reproduce portions of it here. But a few extracts may be given :—

I have been this last half year in Lincolnshire cumbred with concerns amongst my relations till yesterday when I returned hither [Cambridge] ; so that I have had no time to entertain Philosophical meditations or so much as to study or mind any thing els but country affairs. And before that I had for some years last been endeavouring to bend myself from Philosophy to other studies in so much that I have long grutched the time spent in the study unless it be perhaps at idle hours sometimes for a diversion ; . . .

I am glad to heare that so considerable a discovery as you made of the Earth's annual parallax is seconded by Mr. Flamstead's Observations. In requital of this advertisement, I shall communicate to you a fansy of my own about discovering the earth's diurnal motion . . . and therefore it will not descend the perpendicular AC, but outrunning the parts of the earth will shoot forward to the east side of the perpendicular, describing in its fall a spiral line ADEC, quite contrary to the opinion of the vulgar who think that if the earth moved, heavy bodies in falling would be outrun by its parts, and fall on the west side of the perpendicular.

NOTE B.

THE SAVILIAN PROFESSORSHIPS.

The two Savilian Professorships of Geometry and Astronomy were endowed by Sir Henry Savile, Warden of Merton. He was the first commoner to take such a step at either Oxford or Cambridge, though his excellent example was quickly followed by others. The list of occupants of the chairs is as follows :—

SAVILIAN PROFESSORS

Geometry.	Astronomy.
1619 Henry Briggs (Cambridge).	
	1621 John Bainbridge (Cambridge).
1631 Peter Turner.	
	1643 John Greaves.
1649 John Wallis (Cambridge).	1649 Seth Ward (Cambridge).
	1661 Christopher Wren.
	1673 Edward Bernard.
	1691 David Gregory (Edinburgh).
1704 Edmund Halley (Astronomer Royal 1720–42).	
	1709 John Caswell.
	1712 John Keill (Edinburgh).
	1721 James Bradley. (Astronomer Royal 1742–62.)
1742 Nathaniel Bliss. (Astronomer Royal 1763–5).	
	1763 Thomas Hornsby.
1765 Joseph Betts.	
1766 John Smith.	
1797 Abram Robertson.	1810 Abram Robertson.
1810 Stephen Peter Rigaud.	1827 Stephen Peter Rigaud.
1827 Baden Powell.	
	1839 George Henry Sacheverell Johnson.
	1842 William Fishburn Donkin.
1861 Henry John Stephen Smith.	
	1870 Charles Pritchard (Cambridge).
1883 James Joseph Sylvester (Cambridge).	
	1893 Herbert Hall Turner (Cambridge).
1897 William Esson.	

NOTE C.

THE ACADEMIC GREGORIES.

It is natural to feel an interest in the man who was preferred to Halley in 1691; and those who would learn more of David Gregory and the remarkable family of which he was a worthy scion may be referred to a charming little book with this title—*The Academic Gregories*, by Agnes Grainger Stewart (Famous Scots Series: Oliphant, Anderson & Ferrier, Edin-

burgh and London). 'From the middle of the seventeenth century to the middle of the nineteenth century, with a gap of only a few years, some of the Gregorie connection were professing either mathematics or medicine in one or other of the Scottish universities. Galton . . . considers that the mathematical power came into the family with Janet Anderson, who married the Rev. John Gregorie . . . in 1621. From these two are descended no less than fourteen professors.' The Gregorian telescope was invented by James Gregorie (1638–75) in 1663, afterwards Professor of Mathematics at St. Andrews (1669–74) and Edinburgh (1674–5). The David Gregory (1661–1708) preferred to Halley was a nephew of this James, eldest son of his brother David (*not* a professor). What chiefly distinguished David Gregorie was his appreciation of Newton's ideas. It was his object to bring down the *Principia* to the average level of mathematical minds, and both he and his brother James, who held the corresponding chair at St. Andrews, were teaching Newton's philosophy before it was taught at Cambridge.

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